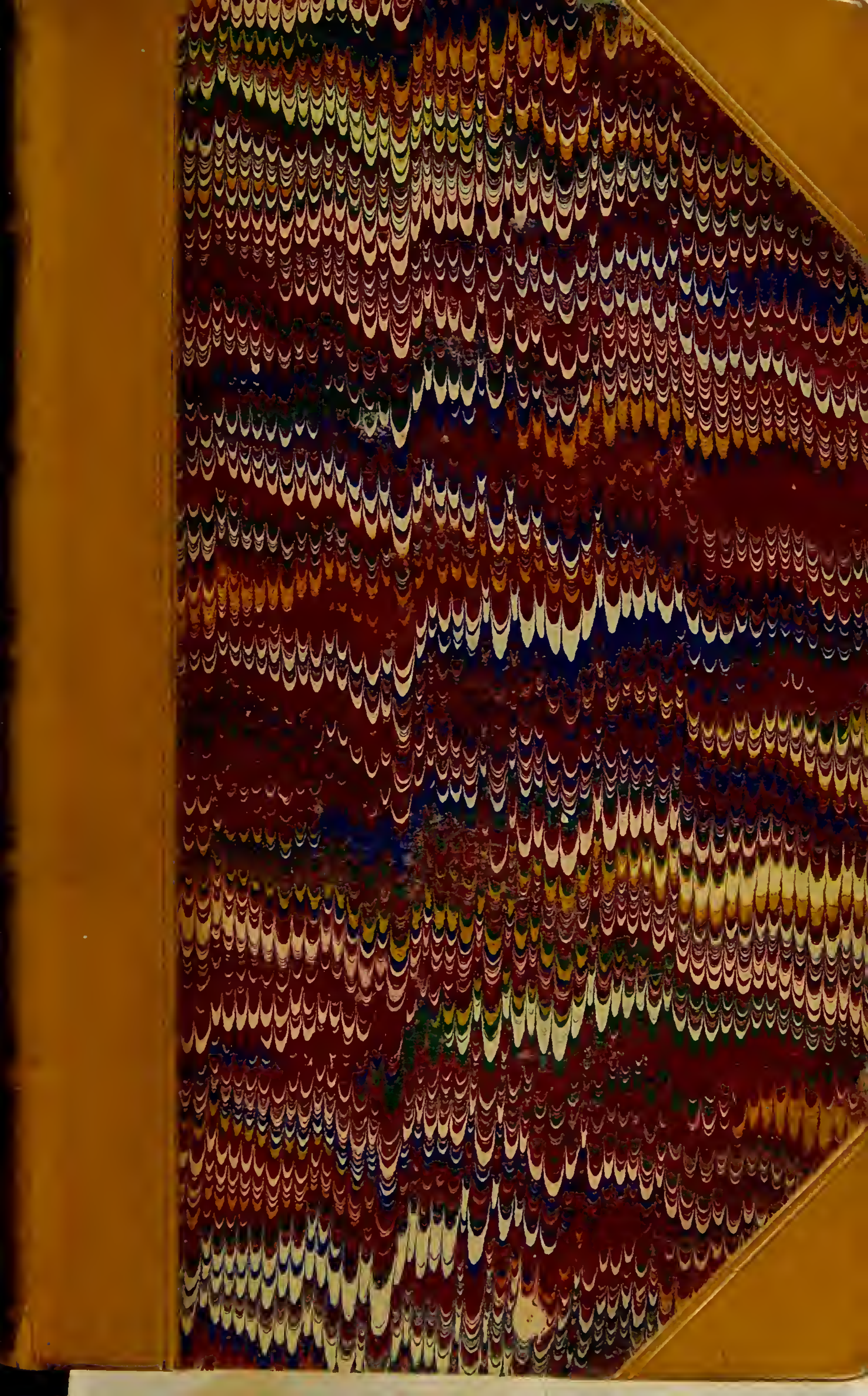

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AN ESSAY ON FOOD.

AN

ESSAY ON FOOD,

IN WHICH

THE RECEIVED DOCTRINE OF MODERN PHYSIOLOGISTS RESPECTING THE *WASTE OF THE BODY* IS EXPLODED—THE CAUSE OF ANIMAL HEAT IS EXPLAINED UPON *NEW PRINCIPLES*—THE SOURCE WHENCE NITROGEN IS DERIVED BY HERBIVOROUS ANIMALS IS ESTABLISHED —GENERAL RULES FOR THE PRESERVATION OF HEALTH ARE LAID DOWN — AND THE WISDOM OF THE DIVINE ECONOMY IN ALL IS VINDICATED.

BY W. GRISENTHWAITE.

AUTHOR OF A REFUTATION OF PAINE'S " AGE OF REASON" : — " NEW THEORY OF AGRICULTURE :" " ESSAY ON GENIUS," &c.

Quantæ res hominibus a Deo, quamque eximiæ tributæ sint.—
Cic. de Nat. Deor.

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MDCCCXXXVIII.

T. C. JOHNS,
Red Lion Court, Fleet Street.

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DEDICATION

TO THE

EARL OF LEICESTER.

My Lord,

If I were asked, why I dedicate this Essay to your Lordship—the subject matter of which is so foreign to your Lordship's pursuits—my answer is this. Your Lordship's life furnishes the best comment, with which I am acquainted, upon the doctrine contained in it. Blest with a princely fortune, and provoked, daily, by the solicitations of a bountiful table—spread for purposes of hospitality—you have learnt and practised that temperate enjoyment of them, which has repaid you with a long and happy existence. The flame, under such moderation, has burnt brightly and steadily. It has never been fouled with what Galen called, the "fuliginous" vapours of intemperance. And they who desire your Lordship's happiness, must imitate your self-denial.

But though temperance ever sweetens, and always prolongs life, it cannot give it perpetuity. There is a time "appointed unto all men." The indisposition which has, lately, visited your Lordship—in your almost patriarchal years—is but the voice of a kind Providence whispering that you are mortal. I well know what gratitude to heaven will fill the hearts of all around Holkham at your recovery. Let any one visit that delightful village—and it is worth a journey from the remotest part of the kingdom—and behold its comfortable cottages; its cultivated gardens; and the air of content that smiles in every countenance—and he will think you was born to be only a Benefactor of the Poor. Let him look at your ample domain and be told what it was when it came into your Lordship's possession—*then* almost a barren waste; *now* converted into a lovely garden. Let him turn to the records of your Lordship's agricultural labours—your princely patronage of the plough—and he will confess, that you have been a Benefactor of mankind—whilst such terms of affection ever accompany the mention of the name of Coke, as I never heard used of any other man. All this I know; and a thousand vouchers would, if necessary, rise up in an instant to attest its truth. Whatever honors may, hereafter, accumulate round the title of Leicester, the name of Coke can never be forgotten.

My fervent prayer is, that when the last drop of the vital lamp shall be expended—as in the course of Nature it must be—your Lordship may be found in the number of those who shall shine for ever, "like the Sun in the firmament."

I do not expect your Lordship to read this

Essay, though, if you should, I am sure I shall stand at a candid Tribunal. My only wish is, that the benevolence which characterises your Lordship's mind, may be found in those who do read it. If it answer no other end, at least, it has furnished me with an opportunity of acknowledging many kind and condescending attentions I have received from your Lordship, and of once more assuring you how sincerely, I am,

My Lord,

Your Lordship's most faithful

and obliged Servant,

Wm. Grisenthwaite.

Manor-House, Beckenham, Kent,
August 7, 1838.

ON FOOD, &c.

There are no two functions of more importance to animal life, than those of the stomach and the lungs; food and air being essential to its continuance, whilst every other process going on in the body may be regarded as subordinate to them; as secretion, assimilation, excretion, exhalation, &c.; and, if we carry our inquiries a little further, we shall discover, that the use of air—as employed in respiration—is but ancillary to that of food; for, after it has performed its office in the lungs, it is *wholly* expelled from them; whereas the food, after having undergone certain changes in the stomach, and afterwards in the lungs, is destined to furnish matter for glandular secretion; at certain periods to promote growth; and, under particular exigencies, to repair injuries and waste; though another, and, hitherto, little regarded use appears to me to be the chief design of it—I mean the preservation of the temperature of the body.

I am not ignorant of the fact, that most writers on Physiology refer the origin of animal heat to the act of respiration; but I have met with no one yet that considers this to be the *almost sole* use of *food;* no one who considers food as even subsidiary to that end; but all of them regard it, and speak of it, as ministering to nutrition, assimilation, and reparation of waste. If I establish the fallacy of that opinion—if I shew that, except during growth, or after injuries sustained by the body, there are no such operations in the animal economy as nutrition, assimilation, or reparation of waste—it will then appear, that, at the present day, there is prevalent a great error respecting the uses of food; and, consequently, respecting the function of those two most important organs of the body, the stomach and the lungs.

I need add little here, to show of what importance it is to be acquainted with those functions; to know, determinately, the offices they have to perform, in order that we may be able to preserve them in healthy action; restore them to it when disordered; and correct any irregularity that may at any time appear in them. But as I am not a medical man, I wish it to be understood, that I write this Essay for those who are also not medical men; but who, like myself, take a deep interest in such speculations as these, and who feel a sincere pleasure whenever they see principles of science made applicable to explain the phenomena of life.

In this short Essay, I also purpose to show, that the preservation of the temperature of the body will explain the manner in which *all the food taken into it is expended*, and, consequently, shall explode the opinion prevailing at present among Physiologists, respecting nutrition, assimilation, and reparation of waste. In the sequel, I shall just glance at electrical development, which may, probably, be referred to the changes that take place in the arrangement of the elements of food before its elimination from the body. This may serve to direct our attention to the origin of physical energy—one of the most striking consequences of receiving food—and, perhaps, throw some light on the mysterious subject of animal magnetism, which at present engages so much of the public curiosity. Let me here, however, entreat the reader not to confound what I undertake to demonstrate, with what I offer upon hypothesis only.

After the elapse of so many ages, after so much laborious research into the functions of animal life, as has distinguished philosophical inquiries of almost every nation, it might have been supposed impossible to advance any thing new on this most important subject, which is, commonly, the first that is discussed by writers on Physiology; and yet but little progress appears to have been made since the earliest period; as will be evident by collating the terms by which food was at first designated, with the opinions which still prevail respecting it—of which opinions they are expressive. Food, at the present day, is called aliment, nutriment, sustenance, &c.; words which convey to the mind ideas of assimilation and support—the supply of new matter, by which the body is prevented from falling into decay. The daily demand for such large quantities of it, as we all know to be indispensably necessary for the preservation of the health of the body, and the emaciation and debility which ensue when this quantity is deficient, very naturally led to the supposition, that such continued supply was needful to repair waste. No other obvious explanation offered itself, especially after the cessation of growth had ceased to make any demand for the augmentation of the body. The same opinion is still currently received, and everywhere promulgated, by writers on Medicine and Physiology, as the following quotations will abundantly testify; in all which, it may be remarked, there is no mention, nor any intimation, that food is designed to preserve the animal temperature.

Dr. Gregory, in his *Conspectus Medicinæ*, speaking "de concoctione alimentorum," says, "Corpus variis actionibus, tum animalibus, tum vitalibus, *brevi exhauritur*, fluidis nimirum partibus dissipatis, *solidis attritis*, et utrisque forsitan ad putredinem sponte vergentibus. Monemur et incitamur importunissimo appetitu, et dirigimur instinctu quem juvat gustus, et olfactus, et experientia ad res ingerendas, quales idoneæ fuerint *ad corporis jacturam reparandam*, ejusque corruptionem arcendam et corrigendam." If I had no other authority besides this to show, that the present opinion of medical men respecting the use of food is, that it serves to repair waste—

"*ad corporis jacturam reparandam*"—it would be sufficient for my purpose; as a writer, undertaking to present to the public a compendium of medical knowledge, must be presumed to adopt the prevailing opinion.

By "actionibus animalibus," Dr. Gregory probably meant muscular exertion; and by "actionibus vitalibus," the action of the heart, the lungs, &c. How the former occasion waste, he has left us to conjecture; and from the latter, I shall prove that none takes place as regards the body. I shall not stop here to comment upon the notion, that food is designed "ad corporis *corruptionem* arcendam et corrigendam," further than to observe, that most food approaches very near to the state here called "corruption"—it has gone through one suite of changes in the stomach, and another in the lungs, which bring it nearer to that state—before it ministers nourishment to the body, or performs any of the offices which he assigns to it; and some medical men have even recommended such a state as more favorable to nutrition. Dr. Pringle first noticed, that a mixture of animal and vegetable substances promoted their mutual decomposition, and, therefore, considered such a mixture as more suitable for food: and such a decomposition is, very evidently, analogous to what is called by Fourcroy, the "putrefactive fermentation."

But this opinion of Dr. Gregory respecting the reparation of waste does not stand alone. Dr. Rees, in his *Cyclopædia*, article, Digestion, observes, "The want of food arises from the necessity of obviating the *losses* which our *body is constantly suffering* in the performance of its various functions, 'tum animalibus, tum vitalibus,' and of preventing, by the *reparation of these losses* (the very words of Dr. Gregory translated, ad corporis jacturam reparandam), the fatal effects they would otherwise occasion." The identity of the opinion here declared, with that just quoted from

Dr. Gregory, and the unequivocal clearness of expression used in both, are sufficient to establish the certainty of the doctrine which is now received.

A modern writer on aliments, says, "Aliments are substances, which being received into the bodies of organized beings, promote the growth, support the strength, and *renew the waste* of their systems." Dr. Thompson remarks, that "the *bodies* of animals do *not remain stationary*, they are *constantly wasting*. Hence the bodies must receive, from time to time, *new supplies, in place of what has been carried off. The use of food answers this purpose.*" Richerand says, "By aliment, is meant whatever affords *nutrition*;" whilst Magendie arranges the aliments which nourish man, according to "the immediate *principles* which *predominate* in those compounds"—beginning with those which furnish three elements only of the body, and ending with those which contain nitrogen also—the peculiar characteristic of animal substances—the last of which he regards as most *nutritive*, because those "principles predominate" which can minister most to its waste—for by nutrition, without going back to the classical derivation of the term, nothing else can be understood, than assimilation during growth, and reparation of waste when growth ceases.

From all these authorities, which might easily be multiplied tenfold, it is evident beyond question, that the prevailing opinion among Physiologists is, that the solid materials of the body are constantly wasting; and that it is the chief design of food to repair this waste. I have thought it proper to establish this point distinctly; lest it might be imputed to me, that I was combating shadows of my own creating. —And as I have no other motive for writing this Essay, as regards others, than an endeavour to throw light upon a subject which involves the health, and, consequently, the happiness of mankind, I hope to avoid provoking the displeasure of medical men, who are sometimes unwilling that any should intrude into those mysteries which, like sacred things of old, they consider to be placed under the immediate guardianship of the profession.

Let us now consider the evidence which is brought forward to support this doctrine of waste and reparation. The first is, the daily demand for food. But this fact I admit, whilst I deny the conclusion drawn from it. *It is the problem I purpose to solve.* The next is, that if food be not taken in sufficient quantity, the body wastes. But this also I admit, whilst I deny the inference, that food is designed to repair such waste—the explanation will be given in this Essay. Thirdly, the deposition and re-absorption of certain coloring matters given with the food, as madder, &c.; though these prove nothing more, than that whilst the circulating fluid is loaded with such coloring principles, they impart them to the *bones;* and that when the same fluid has purged itself of them, they cease to be deposited; for the remarks of Hunter seem to negative the opinion of re-absorption. He observes, that the bones of an animal fed upon such substances, *at intervals*, are *striated;* that it is colored by them when so fed, and free from it when the coloring substance is withheld. But this discoloration of the bone, surely, cannot be urged either as a proof of repairing waste, or assimilation, since the coloring matter is *wholly distinct, in its nature and elements, from the bones in which it is found.* It can add nothing to their substance; and if it be said, that it is deposited at the same time with the fresh phosphate of lime and gelatine, which compose the bones, it is an affirmation without any evidence. If it prove any thing, it proves that there is no circulation of blood through the tabular part of a bone—their removal by absorption always begins at the surface. To this subject I shall advert again in the sequel of this Essay; suffice it, therefore, to remark here, that all the experiments made with madder, were made upon *growing* animals; and from the striated appearance which Dr. Hunter observed in the bones of such animals, when fed, alternately, on food containing madder, and food free from that substance, it would seem that there was but little disposition to absorb it, when once it was united to the phosphate of lime.

But, independently of all close investigation into the structure of the body, or the functions of its several parts, we might infer, *a priori*, from general reasoning alone, that this doctrine of waste is altogether a fallacy.—What should we think of a piece of mechanism, weighing only one hundred and forty pounds, that required two pounds of the materials of which it is composed, every day, to repair waste?—what example can be found among the most fragile productions of art, to approach this dissoluble state of animal machinery? —Shall man, admirably organized as he is, and provided as he is by wonderful contrivances to give perpetuity to his structure, be a work that is to be renewed every ten weeks? If we examine his several parts, we find not one rough action in his whole system. The bones, which we might suppose most liable to injury, move upon each other, with an ease that no art of man can equal; and to shew that easy motion was designed, and, therefore, preservation was intended, every articulation is lubricated with a peculiar fluid. If we look at the muscles, they are composed of strong elastic fibres, and are surrounded by vessels soft in themselves, and filled with fluids—and so resistive are all these of change, that a bone may be exposed for hours to boiling water without sensible waste; whilst Hatchett, macerated "a quantity of lean beef—muscular fibre—in water for *fifteen days*, changing the water every day, and subjecting the beef to *pressure* at the same time. It was *afterwards boiled* for *five hours every day, for three weeks*, in water which was changed every day." And after all this exposure to *maceration*, *heat*, and *pressure*, the fibre still remained; and yet modern physiologists would make it as soluble as lump sugar. In the course of this Essay, I shall endeavour to vindicate the durability of the animal structure; and to show that man is not the *worst*, but the *most perfect* piece of mechanism with which we are acquainted. It is true, that to other excellencies it possesses, it adds, in many cases, that which is peculiar to itself, a power of self reparation, when injured.

If in this Essay I prove, that *all* the food which enters the stomach is expended in *another* way than that of repairing waste—if I show that so far from there being any portion applicable to such purpose, there is some difficulty in finding a sufficiency for *other* uses which *demonstrably* demand it; may I not ask, whether it would not be the grossest violation offered to reason and common sense to maintain still, in opposition to such proof, that food is required "ad corporis jacturam reparandam"—to repair the waste said constantly to be going on in the body? no one can deny it. And now, therefore, to the proof:

From the *mean* of several experiments made by Lavoisier, Seguin, Davy, Dalton, Berthollet, Menzies, Priestley, Allen, and Pepys, &c. &c.—authorities not to be despised—it appears, that during ordinary respiration for twenty-four hours, the quantity of oxygen gas consumed, amounts to about 40,000 cubic inches, and that the *same* volume of carbonic acid gas is respired. Though some variation happened in the course of these experiments, and some discrepancies appeared in their results, yet later experiments, and a revision of the circumstances attendant on those formerly made, have served to confirm the conclusion above stated, viz., that about 40,000 cubic inches of oxygen gas are combined with carbon during every twenty-four hours' respiration. This is one of the data upon which I found my conclusion—a datum not to be questioned; for should it be proved that a little more or a little less carbonic acid is formed daily, such a correction would not, in the least, affect the reasoning of this Essay; it would only require a correspondent correction in the quantity of food necessary to support life.

According to the Atomic Theory, or the Theory of Definite Proportions, carbonic acid is composed of two atoms oxygen, and one atom carbon; or, by weight, of two parts oxygen, and 0.75 parts carbon. Now, 40,000 cubic inches of carbonic acid gas will, upon calculation, be found to weigh about 18,600 grains; of which, according to the above theory, 5070 grains are pure carbon; or rather more than eleven ounces and a-half, avoirdupois weight—all of which are derived from the blood in the course of twenty-four hours' respiration. This is an incontestable fact; at least if any confidence is to be placed in the experiments referred to; and if these experiments should be found to require any correction, we may presume, as I have just mentioned, that it will not be of such a nature as materially to alter the above result; especially when we consider that those experiments were made by so many persons independent of each other, and all of them distinguished for their ability to conduct such investigations.

Before we proceed to consider any other demands upon the food introduced into the system, let us inquire what quantity of it would be necessary to supply these eleven ounces and a-half of carbon; for it is manifest, that the body itself could not sustain such a loss for any great length of time. In about ten weeks, or a fraction more, a man, weighing one hundred and forty pounds avoirdupois, would "vanish into thin air," or be reduced to vapour; for in that period all his carbon would be consumed by respiration; and we shall not stop now to speculate upon the appearance he would make when composed of oxygen, hydrogen, and nitrogen only; even if we left him his "bones marrowless" and ungelatinized—

though I shall, by and bye, put in requisition all the oxygen, hydrogen, and nitrogen, that enter into his food.

If we suppose the food to be *wholly farinaceous*, consisting, for example, of wheat flour, whose chief ingredient is starch—and we may on this occasion consider it as entirely composed of that substance, for the gluten, &c. contained in it would very little vary our conclusion—then, from a *mean* of the analyses of starch made by Berzelius, Gay Lussac, Tenard, and Saussure, we shall find that about twenty-six ounces, *perfectly dry*, will be required to furnish eleven ounces and a-half of carbon. If the substance taken as food had been potatoes, we should have found, by calculations applied to the experiments of Einhoff and Skrimshire, that about six pounds and a-half would yield the same quantity of carbon. Hence, if respiration were the only way in which food is consumed, we perceive that it would need twenty-six ounces of flour—equal to about thirty-five ounces of bread—

or six pounds and a-half of potatoes, every twenty-four hours, to supply it.

By a Letter recently published in the Times Newspaper, it appears, that the Coolies, transported from the East to the West Indies, as labourers, are allowed, daily, one pound and a-half, or twenty-four ounces of rice—a *perfectly dry substance*—about two ounces of ghee (butter), besides a trifling amount of condiments. Rice, says Dr. Thompson, seems to agree very closely with barley; and barley, *as far as our present inquiries extend*, resembles wheat. That is, its elements are so similar, as by their difference not to influence our calculation. The chemical physiologist will understand this, and he who is not conversant with chemistry will understand little of this Essay beyond the conclusions drawn in it. Thus then, it seems, that these poor, expatriated Indians are allowed food barely sufficient to furnish the carbon which is required for respiration only: when we come to consider the amount of recrementitious matter, the renal secretion, and perspiration, the quantity will be found to be below the requirements of nature; unless nature, somehow or other, make up the deficiency—and in the sequel of this Essay I shall endeavour to explain in what way a part of this deficiency is supplied. Still the facts we have now before us—the consumption of carbon by respiration, and the quantity contained in the food taken—so far from favouring the doctrine of waste—furnish incontestable arguments in opposition to it.

Mr. Wood, whose corpulency has been often mentioned, and with whose diet we are acquainted, was enabled to keep himself down to a moveable size by living upon "one pound of biscuit and a pint of skimmed milk a day;" equal to about eighteen ounces of solid food. But this solitary case can hardly be cited to establish a principle; although I think it not difficult to explain what circumstances in his economy might render so small a quantity sufficient for the sustentation of his body; whereas such an enormous mass of carcase might, from its own weight, whether erect or recumbent, have been thought to work its own ruin; and ought, according to the theory of waste, to have received a proportioned quantity of food to repair its losses.

The well known instance of the Marquis of Cornaro presents us with still more remarkable facts. He lived in health, possessed of great elasticity of body and mind, to almost a century of years, upon twelve ounces of solid food a day! To these cases our attention will be again drawn in the subsequent part of this Essay, when an explanation will be offered of the causes to which may be attributed the enjoyment of health under so spare a diet.

From a gaol allowance, now before me, I find that twenty-eight ounces of bread, and two ounces of cheese, per day, are given to those in confinement: a quantity that very nearly approaches the result stated above: viz. which thirty-five ounces of bread would just afford the carbon needed for respiration. But the same difficulty will be found here as exists in the case of the Coolies, when we come to consider the other substances eliminated from the body. The fact of the continuance of life and elaboration of animal substances, under such circumstances, may open a view in physiology which has never yet been regarded. I must, however, reserve my observations till I come to the consideration of those substances. As I know of no other examples of persons subsisting almost entirely on vegetable food, excepting the Brahmins and the poor Irish, and as I have no opportunity of ascertaining what quantity they consume, I shall pass on to the consideration of animal food, which will also direct us to other sources of waste beside that of respiration.

Of animal food, if we suppose it to consist wholly of fibrina, or muscular fibre (and it would not lead us to any material difference of conclusion if we were to regard it as also containing a portion of albumen, gelatine, &c.), we shall find that twenty-one ounces and a half of that substance, *perfectly dry*, will be required to furnish the eleven ounces and a half of carbon carried off by respiration. But muscular fibre, as found in meat offered for sale, loses, at least, two-thirds of its weight on drying. In some instances I found a perfect exsiccation reduce it to one-fourth of its original weight. Hence it appears—if we take the mean of these results—that something more than four pounds and a half of fresh meat would be necessary to afford eleven ounces and a half of carbon. This calculation is founded upon the analysis of M. M. Gay Lussac, and Thenard; and the result is, certainly, one at which we should not have arrived by anticipation. Still, with whatever difficulty it may seem, on a slight view, to be encumbered, it is undeniably true—a fact which no one can question—viz. that if a man respire eleven ounces and a half of carbon a day, he must take four pounds and a half of meat in that time, or his system must gradually waste to supply it. There is no escape from the dilemma—no imaginable source whence it can be derived but the food he receives into his stomach. For if it be supposed that there is an imbibition of it by the skin from the atmosphere, then, were man to breathe at every pore of his body, he would have to deprive about *five thousand gallons* of air of its carbonic acid for *every ounce* of it he absorbed; supposing neither oxygen nor nitrogen, with which it is mixed, to be taken up with it! But this carbonic acid cannot be *thus* derived, because we know that the oxygen inspired, is not returned in *the state of oxygen*, which, *under such circumstances, it would be, but in combination with carbon*, forming carbonic acid gas.

With this statement of *facts* I might leave the difficulty to be explained by others; as no conclusion at which they could legitimately arrive, could, possibly, disturb that which is here drawn; viz. that since eleven ounces and a half of carbon are abstracted from the system during the act of respiration for twenty-four hours, and four pounds and a half of fresh meat just furnish eleven ounces and a half of carbon; therefore, *all* the carbon of four pounds and a half of fresh meat is necessary to sustain such loss by respiration.

We have here no hypotheses except the one above mentioned, viz. the quantity of carbonic acid gas actually formed during twenty-four hours' respiration. I have here, as above, called this result an hypothesis, although its truth rests upon *experiment.* The hypothetical part of it is, whether a man makes the number of respirations, per minute, which these authors suppose—whether the average of the respirations of a whole day would equal, in volume, the quantity found by experiment; and whether the quantity of carbonic acid gas evolved be uniform. But should all these need correction, as I think they do, still our conclusions are proximately true; and that of the use of food in respiration absolutely so; no correction can influence that.

Some part, however, of our surprise will vanish when we consider the quantity of animal food required by carnivorous animals, whose temperature does not differ greatly from that of man. Thus a lion, for example, is said to consume, daily, about fifteen or sixteen pounds of meat, or a quantity about four times that just assigned for the support of man; and if we consider the relative dimensions of the two animals, this quantity will not be found to differ greatly from such proportion. I might add, that in the lion, the cooling surface does not increase proportionately to his weight; as is well known to mathematicians; and, therefore, the quantity of food needed to preserve a given temperature, ought to be proportionably less as the weight increases. But whether my doctrine be true, or that which I am attempting to explode, viz. "the reparation of waste," no cause can be assigned why the *waste* of the body of a lion should, *more than four-fold,* exceed that of a man; and since we do not regard sixteen pounds per diem as extravagantly great for the former, I can see no reason why we should think four pounds and a half too great for the latter.

One reason why animal food is supposed, and, indeed, is known more to invigorate the system than a vegetable diet, is, its more ready conversion into chyle than that of farinaceous matter; as has been proved by Phillips and others; and hence carbon is furnished more speedily for respiration. Another is its production, or formation of less water to dilute the blood; whilst the strength of a buffalo, or an ox, shews, that vegetable food can impart as much muscular energy to the system, as that which is composed of animal substances.

In order, however, to adapt our reasoning to the actual condition of man, let us now consider what quantity of *mixed* food, that is, food composed of vegetable and animal substances, would be sufficient to supply these eleven ounces and a half of carbon.

In an hospital for the reception of the *sick*, whose diet table is now before me, I find, that sixteen ounces of bread, and twelve ounces of potatoes, and an ounce of butter (which, considered *chemically*, may be classed with vegetable matter), constitute the amount of vegetable food allowed to each man per diem. These are equivalent to about seventeen ounces of *dry flour*, as regards carbon. The animal food, per diem, taken from the average of a week, is five ounces and a half: the broth and tea may be regarded as diluents, since it is known, that one ounce of gelatine, will render one hundred ounces of water concrete, or one hundred and fifty ounces tremulous; the last of which is, probably, quite equal to the broth. Now the seventeen ounces of vegetable food would furnish about seven ounces and a half of carbon; and the five ounces and a half of animal food would barely afford another ounce; making the whole quantity equal to eight ounces and a half; or, in round numbers, two-thirds of the quantity said to be expended in twenty-four hours' respiration by a man in full health.

It cannot but have been perceived from what I have already said, that I consider the quantity of carbonic acid, affirmed to be produced in twenty-four hours, as excessive. It is a result founded upon the assumption, that a man breathes twenty times in a minute, and that all his expirations average, in the same time, twenty-eight cubic inches of carbonic acid gas. I find, that I do not breathe, when unexcited by exercise, more than fourteen times in a minute, and, during sleep the number is probably fewer; so that if *respiration only* were to be provided for, I should subsist very well upon this hospital allowance; and I am quite sure that the quantity of food I consume is not more than two-thirds of that which is allowed in it.

But, hitherto, we have taken no notice of the fœcal recrementitious matter, the renal secretion, and perspiration, or whatever passes off by the skin. The first of these, Haller says, amounts to five ounces daily, or, when exsiccated, to an ounce and a quarter. Thompson, quoting from Gehlen's Journal, observes, that it loses three-fourths of its weight on drying. From Berzelius we learn, that this matter is composed of nearly the same substances as those which constitute food; for the bile, which it contains, if that substance differ a little in its qualities from those taken into the stomach, yet in its elements it is the same; and the whole quantity does not amount to one per cent. We may, therefore, without any risk of error, add one ounce and a quarter to the quantity of food above stated as lost to the system on this account.

The next considerable loss is from the secretion made by the kidnies. The amount of this has been variously stated by different writers; and it is probable, that the actual quantity varies as much as their statements. The accounts of Hartman, Robinson, De

Gorter, Keil, Rye, Home, Sanctorius, and Linings, give us the following numbers of ounces in twenty-four hours; 28, 31, 36, 38, 40, 44, 50½, 64; the average of the whole is 49 ounces, or little more than three pounds. According to Berzelius this fluid contains, in every thousand parts, 933 water; and 47 parts, consisting of urea, free acetic acid, lactate of ammonia, and animal matter soluble in alcohol; so that if we suppose three pounds to be secreted every twenty-four hours, the quantity of matter, thus abstracted from the blood, will amount to about two ounces and a quarter: and it may be observed that every part of it, except the free acetic acid, and the lactic acid united with ammonia, contains nitrogen, the distinguishing element of animal matter. Berard has observed, with great probability, that this is the grand outlet for the nitrogen taken into the system.

From a calculation of the quantity of nitrogen contained in all the substances found in this fluid, I find it to amount to about three hundred and sixty grains; or as much as would suffice to form four ounces of *dry* muscular fibre; or between twelve and fourteen ounces of fresh meat—a remarkable coincidence! and quite sufficient to shew, that the nitrogen of our food is not lodged in the system any more than the carbon; for, as we found all the latter carried off from the lungs, so here we find all the former separated by the kidnies; *not leaving one grain to repair waste!* In fact, there is no part of the food taken into the stomach unaccounted for but the oxygen and hydrogen contained in it; for the additional quantity of carbon which forms a part of the muscular fibre, and which is not needed in the formation of urea, free acetic acid, the lactic acid, and animal matter found in that fluid, doubtless, passes off during respiration. The quantity amounts to about one ounce and one-third of an ounce—or, more correctly, 600 grains—which quantity of carbon is contained in twelve hundred and thirty-two grains of *dry* flour, equal to sixteen hundred and forty-three grains of bread, or about three ounces and three quarters.

If any one should wish to verify the result of Berzelius' experiments, it will be necessary that he should examine the fluid under consideration at different times, as the quantity of urea, &c. it contains, will be found to vary considerably.

Thus, as we proceed in our investigation, how the mystical doctrine of waste is seen to vanish! how exactly do the *injecta* and the *ejecta* of the system balance each other!

I need not consider the bile, since the whole of it is carried into the intestinal canal, and forms, as we have already noticed, a part of the fœcal matter. What escapes by perspiration, except water, is inappreciable in quantity. Acetic acid, an oily substance, and urea have been detected in it. These, except the oily substance, are the same as those found in the renal secretion: and when we remember how the skin and the kidnies alternate in their action, this result is no other than we might have expected. Still we have the oxygen and hydrogen of our food—at least a part of them—upon our hands to be disposed of. Of these, in the first place, the solids in the fluid we have been considering, will be found to contain about two hundred and twenty grains of oxygen; and, taking the mean of the analyses of urea of Prout and Berard, the hydrogen will amount to one hundred and two grains.

Let us now return to the quantity of food taken into the stomach, in order to ascertain the whole amount of oxygen and hydrogen still remaining to be accounted for. This quantity we have found to consist of twenty-six ounces of flour, or substances that resemble it, and about thirteen ounces of animal matter, as muscular fibre; the former being required to furnish the carbon consumed in respiration, and the latter the nitrogen contained in the urea, and animal substances discovered in the fluid secreted by the kidnies; and if the quantity of carbon contained in the animal food, and not required to form the solids found in the renal secretion, is to be subtracted from that expended during respiration—which is probable—then we shall have to deduct two ounces and three quarters from the quantity stated above, leaving twenty-three ounces and a quarter of vegetable food only, instead of twenty-six, with about thirteen ounces of animal food. The former of these contain about 5028 grains of oxygen, and 698 grains of hydrogen; and if the whole of the oxygen were to unite with as much hydrogen as would form water, we should have but 79 grains of hydrogen in *excess*. This calculation is founded upon the mean result of the analyses of Gay Lussac, Thenard, and Berzelius. Had I taken the analysis of Saussure, we should have found a *deficiency* of hydrogen amounting to rather more than 14 grains. It is, therefore, not improbable that these two elements, oxygen and hydrogen, exist in starch in, very nearly, if not exactly, the proportions in which they form water; and if so, we may suppose, when the carbon is withdrawn from it, that water is actually formed. There are many chemical analogies to justify this conclusion. Thus Lignin, when exposed to the action of sulphuric acid, so as to insulate the carbon, is found to leave its oxygen and hydrogen in the state of water; as has been proved by the experiments of Fourcroy and Vauquelin; and the cuticle undergoes the same change—except that it also furnishes ammonia—as I have, repeatedly, experienced.

Here then we have an explanation, quite satisfactory, of the way in which the oxygen and hydrogen of our *vegetable* food is disposed of; viz. either by the lungs, the skin, or the kidnies; from all of which a quantity of water is evolved; and, very frequently, *a quantity considerably greater than the amount of fluid taken into the* *stomach.* I am not ignorant of the fact that the skin will, under particular circumstances, absorb that fluid from the air; but many facts might be mentioned to shew, that a great quantity is supplied by the food. The whole quantity of fluid, of every kind, taken by myself and four children, the youngest fifteen years of age, does not exceed five pints a day. The result of Seguin's experiments—if considered as an average of a whole day's perspiration—is certainly beyond the truth. He states it, at a *maximum*, to be 26,25 grains troy, and, at a *minimum*, nine grains, per minute; or upon the *average* for a whole day, at nearly fifty-three ounces. This, with the renal secretion, would amount to more than six pints and a quarter in the twenty-four hours; or, if that carried off by the lungs be added, the whole quantity would exceed seven pints and a half! This is certainly a fallacy. Whence can such a quantity be derived? Not by the skin, because that, according to his *own view*, is *giving off* fifty ounces a day; unless a double current, no very probable conjecture, like that at the Straits of Gibraltar, is going on. The food cannot supply one quarter of it; and the lungs are doing the same office with the skin, in dispersing it.

That no considerable quantity of *solid* matter is carried off by perspiration admits of the clearest proof. First, because no considerable quantity of solid matter has been detected in it, which, had it existed, can hardly be imagined to have escaped observation; the few nodules of fat discovered by Mr. Cruikshanks on his "fleecy hosiery" cannot be magnified into this importance; and it may be observed, that that gentleman ascertained, that the fluid collected from his hand, confined in a glass-vessel for an hour, "possessed all the properties of *pure water.*" Secondly, what substances that are volatile could escape without *obtruding* themselves upon our notice. Acetic acid, it is true, has been found in perspiration; but a *single ounce* of that fluid is equal to two pints of strong vinegar, which would impart its odour to a very large room; and half a dozen persons sitting in a small apartment, would be as pungent as a pickle-shop; whilst a meeting at Exeter Hall, would exceed the vinegar works of Champion and Co. The same reasoning will shew that it cannot be ammonia; and if to get over the difficulty—since carbonic acid gas is known to be emitted from the skin—it should be contended, that the ammonia is supersaturated with this acid; then arises this other difficulty, viz. that when ammonia is so supersaturated (forming a bicarbonate), it is no longer volatile. But, thirdly, whence could the materials be found to form any considerable quantity of perspirable matter? We have ascertained, that almost every grain of food is required for respiration and renal secretion—we have traced almost every atom of it to its exit from the body; and, therefore, none remains for perspiration. And the body cannot itself furnish it, because it would then suffer a perpetual loss without reparation; for there are no materials remaining with which to repair it.*

I should not have been thus particular in proving that perspiration carries off no appreciable quantity of matter from the body, but to obviate a *conjecture* which unscientific persons might entertain, and do entertain, that perspiration is the cause of great waste; and even scientific readers might raise an objection on that head, since the *actual* quantity carried off by the skin has not been assigned by philosophical writers, as has been done with regard to carbon from the lungs, &c.

Let us now examine the muscular fibre. Thirteen ounces of this substance in a fresh state, or four ounces of it *dry*, contain 362 grains of oxygen, and 131 grains of hydrogen. If from these we subtract the oxygen and hydrogen contained in the solids found in the renal secretion, there will still remain to be accounted for, 135 grains of oxygen, and 54 grains of hydrogen.

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* I must intreat the reader to pause, before he objects to the above reasoning, that the emunctories of the body are known to carry off water, acetic acid, urea, &c.; and, therefore, these must be derived from some source or other. I am equally cognizant of that fact; and shall attempt its explanation in a subsequent Essay, if called for by any interest excited by this.

Having disposed of *all* the *vegetable* food in a way which no one acquainted with science can question, viz. all the carbon of it by respiration—all the oxygen and hydrogen in the vapour emitted from the lungs and skin, or contained in the water secreted by the kidnies—and having shewn, beyond contradiction, that all the nitrogen of the *animal* food is found in the last mentioned fluid; even if these residuary quantities of oxygen and hydrogen could be assigned to no substance known to be eliminated from the system, they might, without the slightest infringement upon our reasoning or conclusions, be wholly disregarded: especially when we consider the comparatively imperfect state of the analysis of animal substances, the difficulty of making accurate experiments upon the gasses evolved during respiration, and assigning the exact quantity of substances contained in the renal secretion.

The oxygen and hydrogen remaining; viz. 135 grains of the former, and 54 grains of the latter, amount to about *one part and a half in the hundred, of* DRY *food taken into the stomach;* an approximation to exactitude almost within the limits of experimental proof. But it may be added here, that no notice has been taken of the oxygen and hydrogen contained in the acetic acid found in the renal secretion, and ascertained to exist in perspirable matter; as well as the hydrogen which forms a constituent part of the oily matter of the latter. If all these were examined—since each contains a portion of the elements now under consideration—there can be no doubt but that adequate uses would be found for the small quantities that remain.

If any one shall, fastidiously, object to the other conclusions, which rest on demonstration, because a *few grains*, out of *many thousands*, cannot, with the *same certainty*, be accounted for, I beg to say, that this Essay is not written for him. I neither claim such a degree of perfection myself, nor expect it in my readers. In the preceding pages, *all* the food taken into the stomach, is shown to be evolved from the system—exactly the same quantity of the same elements—no one is deficient, no one is in excess. If this fact be not sufficient to satisfy every one, that the introduction of the first, is the source of the formation of the second, he must prefer "darkness to light;" and love rather to wander in the mists of speculation—for the sake of a theory—than advance under the sunshine of demonstration. But it may be necessary to follow them a little way in their dark path of hypothesis, in order to point out to them the errors and difficulties into which they are liable to fall.

When the doctrine of waste was promulgated, the necessity of reparation became indispensable; and to make them agree, the one was considered exactly to balance the other. A child would have formed a similar hypothesis; since any inequality between waste and reparation, or of taking away, and adding to a mass, must, evidently, either increase or diminish it. But let us inquire what evidence the advocates of waste, have to advance in support of their hypothesis. How do the *débris* of the system escape from it? Not by the lungs; because no atom escapes thence, but what is furnished by the food, —not from the kidnies; because every particle of that secretion is furnished by the food: and from all other sources the quantity is inappreciable; and even though inappreciable, is needed to explain the manner in which a portion of food is expended. But, then, they may affirm, that all the matter carried off in the ways just mentioned, is but the ruins of the animal structure; and that the elements of food go to repair the damage thus sustained—an hypothesis which is a slander upon nature, and, as we shall shortly see, quite repugnant to principles of science. These theorists say, that the carbon emitted from the lungs is abstracted from venous blood; which, they contend, is loaded with that substance; an opinion which I intend to refute in a future Essay, should this excite any interest in the public mind. But let us, for the sake of argument, allow their assumption to be true; for by this, their own hypothesis, they will be found to fall. If venous blood contain this excess of carbon, whence is it derived? From the waste of muscles, ligaments, &c. they reply. But how, I ask, does this carbon get into the veins? There is no proof that the veins can receive any part from these "*solidis attritis*"—a distinct apparatus is contrived for *that* purpose—the absorbents: and it would be ascribing supererogation to nature, as well as manifest imperfection of contrivance, to assign the same office to two instruments. Frustra fit per plura quid potest fieri per unum.

Well, but still these fragments of the animal machine may enter the veins where the lymphatics join them? True. But then comes this irresoluble difficulty—the blood is venous, has got *all its carbon long before the lymphatics unite with the thoracic duct, or the contents of that duct are poured into the subclavian vein;* so that this fact at once negatives the opinion, that the carbon of the blood is derived from the ruins of the animal structure. Besides, if the absorbents are the conduits of this redundant carbon, why is the fluid in *them* not *black?* In them it ought to be pure ebony. And even if the veins are imagined—to support a fanciful hypothesis—to do the office of the absorbents, still the absorbents, having a *share* in the business, ought to exhibit, at *least,* the *same color.* But if this fact demolishes their unsupported conjecture, we have other evidence that will sweep away every fragment of it that remains. Before I proceed to that evidence, it may be well to remark here, that such must be the fragility of our bodies, according to the theory I am combating, that whether we walk, work, rest, or sleep, it is *almost equally* going to ruins—the same waste proceeds—for man needs, *almost*, the same quantity of food under all these circumstances—that is, when every limb is exposed to violent action—to the wear and tear of hard labour—or when every muscle is at rest. I know that exercise demands more food, but in no proportion to the waste that ought to take place according to these theorists. Wood, the Essex monster, of whom we have already spoken, rode, and worked hard upon a quantity of food not greater than is consumed by many delicate ladies; so that his body could not be subject to waste—he was a perfect Nestor Ironsides. Again, the lion wastes *sixteen pounds of muscular fibre a day!* although confined in a den, where his exercise is almost nothing! But let us now come to the proof above promised. Seguin found, that the *same* exercise which had occasioned the formation of 3200 cubic inches of carbonic acid gas, per hour, by respiration, increased it to 4600 cubic inches per hour, *when taken after a meal;* or one and a half times as much again; so that, according to these theorists, *when he began to eat, he began more rapidly to waste.*

This is quite inexplicable upon such a supposition; but quite agreeable to the doctrine of this Essay; for when he began to eat, he began to supply the blood with more carbon: but *why the structure should go to ruin, half as fast again, after receiving nourishment, admits of no explanation.* Neither does this example of Seguin stand alone. Digestion *always* occasions an increased expenditure of carbon. I could quote twenty authorities to establish it, if every one's own experience did not satisfy him: and to render it more striking, it may be observed, that this increased formation of carbonic acid goes on, whether the animal be at rest, or in exercise—though exercise, doubtless, much augments it—*for not a muscle of the* *body can be moved, but at the expense of a quantity of carbon.* The further prosecution of this view of the subject, I reserve to hereafter.

Can any one still doubt where the truth lies, with these facts before him? Let him review them. A quantity of carbon is consumed in the lungs. *That* quantity, *precisely*, is furnished by the food; and whenever food is taken, at *that moment more* carbon begins to be evolved. The chain is unbroken; we see, as it were, the movement of a link at one end of it, attended by a movement of a link at the other.

Notwithstanding the cogency of these arguments, and the irresistible appeal they make to the unprejudiced judgment, yet mankind will often cling to the last fragment of a received doctrine; and, in the present instance, many, perhaps, will say, if the theory of waste and reparation is to be abandoned, still the certainty of growth and assimilation cannot be denied —the fœtus has not the weight of the child, nor the child the weight of the man; and, therefore, there must be the appropriation of food to favor accretion—all cannot be applied to purposes of respiration and excretion. Here it would seem the advocates of assimilation might stand, and vaunt defiance; but a little closer inspection of the subject will show us, that this fact, so apparently favorable to their views, is most fatal to them.

In the introduction of this Essay, I admitted—what is palpably true—that food, at certain periods of life, is destined to promote growth; by assimilation, if the term please the reader—it is as good as any that could be used, though it signifies no more, than that muscular matter is added to muscle, and bony matter to bone. But it may be said, here are muscles and bones, &c. formed, whilst respiration and secretion are both going on, and going on as vigorously, as before that formation begun; and, therefore, at all events, some portion of aliment, over and above that required for those purposes, must minister to growth. True; and let us now see, in the first place, what quantity is required for this end; and, secondly, with what facility it is supplied.

Let us begin with the *fœtus in utero,* as that seems to be, as regards the mother, something like a new creation of matter. Dr. Hunter states, that of several thousand new-born, and perfect children, which were weighed at the British Hospital, in London, by Dr. Macauley, the smallest was about four pounds, and the largest about eleven pounds and two ounces; but by far the greater number were from five to eight pounds avoirdupois. Dr. Clark found them to vary from four to ten pounds; and the average to be something more than seven pounds. Roeder states the average of his experience, to be six pounds and a-half. To give the advocates of assimilation the benefit of an *extreme* average, we will allow the weight of a new-born child to be nine pounds. Here then we have nine pounds of animal substance formed during the period of nine months; or, supposing it to be uniform, which it is not—though sufficiently so for our present purpose — one pound is produced every month, or four-sevenths of an ounce per diem—a fraction more than *half an ounce daily!* No very great quantity, when we consider that the food of a person, for such a period, is allowed to contain forty-four ounces of the same elements as compose the fœtus—about *one per cent.* only! It is true that a female may be considered to receive something less than this quantity of food; but that will not affect the argument, —we are not now at issue about fractions of an ounce. But this half ounce a-day, small as is the quantity, nature is so little able to spare, that it generally occasions a manifest shrinking, a waste of the muscles of the mother! At the same time, it is notorious, that pregnant women do, generally, and purposely, take more food than at other times; thinking it to be necessary. But, then, it is to be remembered, that the *taking* of food, is not all that nature has to do in this business.—That food must undergo certain changes in the stomach and the lungs before it can minister nourishment to the fœtus; and the stomach and lungs are, probably, employed to very nearly their full function, before this additional demand is made upon them. The formation of the fœtus is not like the increased expenditure of carbon which accompanies increased respiration. In the latter case, the very expenditure occasions an increased solicitation for more food.—It is like directing a blast of air to a quantity of fuel already in a state of combustion; whereas the former is like heaping on fresh fuel, which *needs more air* for ignition.—The one makes the fire burn brighter, and consume more carbon, and, therefore, demand more food—the other loads the stomach with food, or the blood with carbon, which cannot be carried off but by a febrile action —a preternaturally increased respiration.

As this Essay is written for men of ordinary minds like my own, and who stand in need of all helps and illustrations to make some truths appear evident, I will here endeavour, by a familiar example, to render my reasoning plain to them. If a man had a wall so exposed to casualties from winds and water, &c. that he found it daily to sustain a loss of from one hundred to one hundred and twenty bricks — and the quantity of food taken under different circumstances varies more than this—so that he was obliged to be provided with that number for its reparation — would it not appear strange, if he wished at any time to lengthen or heighten his wall, that he could not spare a *single brick a-day* for such a purpose? For the *waste* of a hundred, or a hundred and twenty, he *is* provided; surely the addition of *one* brick could occasion no inconvenience. His resources must be more than sufficient to meet such an exigency. And yet we find the want of one brick—where there is said to be an hundred *wasted* — producing sometimes emaciation, and sometimes febrile disease. Can the animal kiln, which furnishes materials for such enormous *waste*, and *varying waste*, not supply *one* brick, when the wall is to be *extended?* But I might have come to my conclusion at once. Let the advocates of waste, reparation, and assimilation, take this half ounce of matter a-day, furnished to the fœtus, and deal with it as they please. This I affirm, without any dread of contradiction, that if a woman *not* pregnant, consume in respiration, &c. a given quantity of food; then, *during pregnancy*, if she furnish half an ounce of substance to a fœtus, she *must* either *elaborate that additional half ounce of food, or breathe, and secrete less, or become emaciated.* And, after all, it is but about half an ounce of food a-day that we are discussing—but even that half ounce, it appears, nature cannot supply with impunity, subject as she is said to be to waste—to the waste of almost one hundred times the amount here demanded.

Does not the balance between digestion and respiration explain this? When the fœtus is to be formed, a portion of aliment is, obviously, required; and it seems difficult for nature to accommodate herself to this condition. More food passing through the lungs in the same time we always find (as has been already mentioned, and as is well known) to increase the pulmonary action; and to be the only cause of such increase—except disease. But pregnancy is not a disease, though improper diet and regimen frequently make it one. For a woman to be pregnant is as natural as for her to digest her food. An equally admirable apparatus is contrived for both. And if we call to our remembrance the peculiar habit of women—the catamenia—for whose existence no other reason, I think, can be assigned than the design of nature, which, contemplating an extraordinary demand, at certain times, thus provides means for its supply—giving to them a power of generating a superfluity of blood; which, though abridged in a state of refined life, is, probably, just equal to its purpose in a state of nature.*

Let us now come to a few other instances of waste, and see whether they be not corroborative of the doctrine of this Essay. We all know what a slight rheum—an almost inconsiderable secretion from the nostrils—will occasion. If it last a few days only, it makes a sensible impression on the countenance—something of its plumpness is lost. This, probably, arises, in some degree, from the too rapid emptying of the adjacent vessels of their fluids, causing a flaccidity: whilst a fever actually wastes the body by abstracting an excess of carbon from the blood. At such times more blood passes through the lungs to be decarbonized, than the stomach can furnish by digestion. The effect is soon visible. Even if the appetite and digestion were in no degree impaired, waste must happen, because more is taken away than is supplied.—One hundred and forty pulsations in a minute will occasion an expenditure of *double* the quantity of carbon in the same time that takes place in health; or consume daily as much of the substance of the system as is equal to about two pounds of bread, and thirteen ounces of meat. That this really happens is proved by the *increased temperature* of the body; for which we have framed a particular designation, and call it "fever heat." This is not hypothesis, but fact. In the same manner an open ulcer occasions waste, even if all the functions of the body retain their full power; though, doubtless, in many cases, the waste is more rapid from derangement in the offices of the stomach. Again, I know a gentleman who, in twenty-four hours, by a violent diarrhœa, was reduced from full energy of body to extreme debility. Here was waste, not of the body, but the food which should have sustained the body—though, doubtless, the prostration of strength arose from impoverishing the blood; as I shall endeavour, shortly, to prove. How can we suppose such an inconsiderable waste as that alluded to—if it were an abstraction from the muscular fibres—to debilitate the body? The loss sustained did not, probably, amount to a solid *pound.* The gentleman weighed upwards of twenty stone; and the effect of the diarrhœa could not equal one two-hundredths of his weight. He has since decreased two stone in weight, without any diminution of strength. All this admits of easy explanation, without having recourse to the hypothesis of waste. The blood is, to a certain extent, the depositary of matters to be elaborated into carbonic acid, urea, &c. This is proved by the period man can forego sustenance; though the stomach, or rather the intestines, probably, supply it to the blood as needed.

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* It may perhaps be objected, that other animals—for which the above provision is not made—with even greater facility than women, furnish nourishment to the fœtus. But then it is also known, that other animals—when supplied with abundance of food—are all disposed to become fat. This, probably, answers the required end. When gravid this economy—or call it what you will—is made subservient to the nourishment of the fœtus. And, in further confirmation, it may be remarked, that women inclined to obesity, are known to have, generally, fewer offspring.

If food be withheld too long, all that was lodged in the stomach, the intestines, and the blood, is consumed; and debility supervenes. We feel an inanition. But if the doctrine of waste is to be maintained, we should surely be able to support such intervals with less inconvenience, since such waste matter—scarcely a single fibre from each muscle—might furnish enough of materials for respiration and secretion. A few ounces drawn from several thousands, could not occasion all the debility which attends such abstinence; especially since the body, as in the above mentioned instance, may lose ten per cent. of its weight without any loss of strength; nay, probably, with an increase of it; for obesity frequently induces weakness. But, though the body may lose ten per cent. of its weight with impunity, the blood, perhaps, cannot lose one per cent., without producing some debility. This person's blood was, probably, I might, perhaps, say certainly, unchanged at the two periods named; if not in quantity, at least in its elements; upon which depends the vigor of the system.—A reduction in size would, necessarily, be followed by a reduction of blood. The abstraction of a few ounces by venesection, will, sometimes, produce syncope, and always faintness; except when a previous plethora existed.

Here, then, we see the doctrine of *continual* waste and reparation wholly discountenanced. Waste may arise from disease, and reparation be made in health—which latter resembles growth—but the waste here spoken of consists of matter carried off in *excess:* I mean expended in addition to what is ordinarily consumed when the body is sound.

When speaking of growth, I thought it unnecessary to pursue the subject farther than the consideration of the fœtus. Had we extended it from the fœtus to the full-grown man, we should have found, upon the average, an increase to the extent of something short of one hundred and forty pounds. Now, these one hundred and forty pounds of animal matter are not furnished in less than twenty years, seldom in so short a time. This is at the rate of *seven pounds a year;* a quantity considerably less than that required in the formation of the fœtus in nine months! So completely do many stupendous objects, seen in a mist, vanish when we approach, and attempt to handle them.

But I have a few words more to say upon the subject of waste. As far as experience extends, *cæteris paribus*, waste might be presumed to increase with an increase of temperature—almost all chemical changes observe this law. But, in the present instance, we find just the converse to be the truth. Lavoisier speaks of a man who, at the temperature of 54°, consumed 1344 inches of oxygen in an hour; whereas in a temperature of 79°, he consumed only 1210 cubic inches. This might, *a priori*, have been inferred from the preceding reasoning of this Essay; and still more so after what I have yet to advance upon the generation of heat; but is quite at variance to the theory of waste. Even the rarefaction of the air would account for the greater part of the reduction observed. If air expand $\frac{1}{480}$ its volume for every degree of temperature—as it is known to do—then, in the present case, there would be an augmentation of it amounting to $\frac{25}{480}$, or $\frac{1}{19}$. Now $\frac{1}{19}$ of 1344 is equal to about 70 cubic inches, which, subtracted from 1344, leave 1274; and it is possible, that the greater quantity of vapour contained in the air, in the latter instance, would explain away a considerable portion of what remains in excess. It may, moreover, be observed, that respiration becomes more languid as the temperature increases; at least when it approaches 80°, as it did in this case.

Immediately connected with this view of the subject, is the consideration of the food before mentioned, given to the Coolies;

and, indirectly, it will apply to the case of Cornaro, Wood, &c.

We found, that all the food taken by the Coolies would barely suffice to supply carbon for respiration; but then we are to remember the temperature of the climate in which they live. This would be found sufficient to explain a part of the difficulty—the air is more attenuated, and the respiration is more languid. Whence the *urea* was derived, I shall, shortly, endeavour to show.

With regard to Cornaro—if he really subsisted upon *twelve ounces* of solid food a day—it is clear to demonstration, that he could *not* expend *eleven ounces and a half* of it in the formation of carbonic acid—he must have eaten pure charcoal to supply it. He could not respire so much carbonic acid gas as ordinary men—there is no *imaginable* source that could furnish it; though it will be seen, in the sequel, that twelve ounces of food, in a genial climate, like Italy, might, in a somewhat anomalous instance, as this of Cornaro, answer all the purposes that twice that quantity would do in Britain: and it is certain, that in warm countries, the quantity of food taken is vastly less than in cold. It consists chiefly of fruits and vegetables, which furnish less of *dry* matter. Among the Abyssinians, there is a saying, that "princes only eat meat every day." The saying, however, serves to confirm the truth of my observation. What a warm climate does to a whole people, accumulation of fat does to particular persons in a cold one—it creates a climate within them.—This, probably, was the case with Wood: and hence it is, that the Whale sports in the frozen seas of Greenland; and, though surrounded by ice-cold water, has a torrent of warm-blood circulating through his system. It is also an observation, verified by a multitude of instances, that fat men consume less food than those who are spare of habit. Lean, and hungry, are epithets, not poetically only, but familiarly associated. All these anomalies will be found reconciled by my view of the use of food ; to which, therefore, I shall now proceed.

Whatever other purpose food may be contended to answer, this one is certain, that it is the only source of animal heat; and this is true, whether the theory of Crawford, or that of any other Physiologist, be entertained; though, in the course of this Essay, I hope to prove, that the received doctrine concerning animal heat is quite fallacious; and, in a future Essay, shall attempt to prove, that the nature of the blood, and the changes it undergoes in its circulation, are not at all understood. But to return to animal heat. After the theory of animal heat was first formed, as an excellent writer on Physiology observes, "It was objected, and, apparently, with justice, that if the union of the carbon and oxygen took place in the lungs, *their* temperature ought to be *much greater* than that of any part of the body;

and this objection appeared so forcible as almost to induce Black himself to relinquish the theory: when the investigation was taken up by Crawford. The result was the formation of an hypothesis (the readiest and most effective instrument imaginable for solving all perplexities), which seemed to remove the difficulties of Black." This difficulty, however, appears to have alarmed others also. Dumas, in his *Principes de Physiologie*, thus speaks of it. "Si la chaleur vital tirait sa source unique des poumons, elle devrait être *dans ces organes incomparablement plus forte* qu'elle ne l'est par tout ailleurs; et l'activité de ces embrasement, *necessaire pour echauffer tout le corps*, irait a *detruire la substance des parties qui en seraient le foyer*." Lagrange and Hassenfratz have been startled with the same objection. To quiet all these, a different capacity for caloric is assigned to venous and arterial blood; which later experiments have more than called in question. Dr. John Davy, as well as M. M. De la Roche and Berard, deny this superior capacity of heat in arterial blood, and of inferior capacity in carbonic acid gas; at least to any thing like the extent that had been inferred. But, according to Crawford, the heat generated in the lungs is not sensible heat, but heat locked up in the arterial blood, which is let out during the circulation. The thought was certainly ingenious, and only wanted facts to make it credible. Not but that it was still encumbered with great difficulties. First, because, if, on the conversion of venous into arterial blood, the latter absorbed *all* the heat, then *no* heat would be evolved during its circulation through the arteries; for, by the *assumption, it is no hotter than venous blood;* and heat is never imparted except to bodies of a *lower temperature;* so that all the caloric would be set free on the conversion of the arterial into venous blood; which takes place in the capillaries only; and, therefore, a man's hand or foot ought to be hotter than his lungs, or his heart; an opinion which, I think, no one will maintain. Secondly, if the heat generated in the lungs be in *part* rendered *latent*, whilst a *part* is *free*, then—although there would be a better distribution of it than on the former supposition—the contrivance becomes more complicate; for the lungs, and heart, and arteries *would* be exposed to a portion of that increased temperature which alarmed Black, Dumas, &c. whilst still the extremities ought to exhibit a greater temperature than the *abdita* of the body; for in them would be extricated *all* the *latent* heat, and a *portion* of the *free* heat not expended in the course of the circulation. Again, if this extrication of heat occurs when arterial blood is changed into venous, then, in consequence of the less capacity for caloric of the latter, venous blood ought to be *hotter than arterial blood*, which is contrary to fact; for arterial blood is acknowledged to be warmer than venous. But, as some persons do not readily enter into abstract reasoning, it will be as well, for the benefit of such persons, to illustrate what I have here said in opposition to the theory of Crawford, by an example. Suppose venous blood to contain one hundred degrees of heat, and the capacity for caloric of venous and arterial blood to be as ten to eleven—which is very near to Crawford's hypothesis—then arterial blood, exhibiting a thermometric heat of one hundred degrees, would, actually, contain as much caloric as would raise venous blood to the temperature of one hundred and ten degrees; but ten of these degrees would be latent in the arterial blood; and *would remain so till some body of lower temperature than itself was presented to it.* Hence the arterial blood formed in the lungs by respiration, though actually containing ten degrees of heat more than venous blood, would, by the thermometer, appear of the *same* heat. Let us apply what has just been stated to the circulation of the blood. The lungs are now not heated in the least by respiration; for all the heat is locked up in the arterial blood; consequently, the *pulmonary veins* receive no heat—the *heart* receives no heat; for its temperature must, at least, be equal to that of venous blood, since all the venous blood has passed through it to reach the lungs. And whether we pursue the arterial course, upwards or downwards, the same result will appear. The *carotids* can receive no heat; for they are imbedded in substances whose temperature is, at least, equal to venous blood; the *aorta descendens* can receive no heat, for the same reason; and so also we shall see, that the *subclavians*, the *emulgents*, the *femoral*, &c. can receive no heat, for they are all as hot as venous blood. But when we come to the capillaries, where arterial blood is converted into venous, then the ten degrees of heat, said to be latent in arterial blood, should instantly, develope themselves. The less capacity for caloric of venous blood forbids them any longer to be shut up—*its temperature should now rise to one hundred and ten degrees*—the toes and finger ends should glow with heat; and on the return of the blood by the veins the whole system should be warmed. And, when it has again reached the lungs, these ten degrees of heat should be just expended, or there would be a fearful accumulation of it in the body; as happens in fever. Thirty-three pounds of blood must be heated ten degrees every three minutes and a half! This would require the consumption of more than ten *pounds of carbon a day*, instead of eleven ounces and a half! A boiler, holding more than four gallons — the quantity of blood in the system—placed over a large kitchen fire, would not receive heat so rapidly as this. But as I hope to vindicate the simplicity of nature's operations in this instance, as they every where appear, it would be superfluous to urge any further objections to the hypothesis of Crawford. Upon *facts* I set my foot whilst I offer this vindication.

We have seen, in a former part of this Essay, that eleven ounces and a half of carbon, enter into combination with oxygen, during every twenty-four hours' respiration; or almost thirteen grains every three minutes and a half; during which time thirty-three pounds of blood have passed through the lungs. Now thirteen grains of carbon—as is well known to those acquainted with the generation of steam—would, on combustion, or combination with oxygen to form carbonic acid gas, convert one hundred and four grains of water, at 212°, into vapour. This, according to Buchanan, takes place in a well-arranged furnace and boiler. We may, therefore, safely consider, that quite one hundred and forty grains of vapour, at least, would be formed, if no heat were lost by radiation; and in the lungs none is so lost. To this must be added the heat contained in the air which supports ordinary combustion, amounting to ten pounds for every pound of coal consumed; all of which escape at 300° or 400° of temperature; sometimes considerably higher, as appears by the spontaneous combustion of the carburetted hydrogen, and carbonic oxide at the top of steam engine chimnies. Here, then, we have one hundred and forty grains of steam, containing a thousand degrees of latent heat, or as much as would raise one pound and a half of water fourteen degrees and a half, or thirty-three pounds of water *about* three quarters of a degree ; allowing no part to be lost in the air respired ; which is inconsiderable. Now, supposing the blood to have the same capacity for caloric as water—which is very near the truth—then we perceive, that the thirty-three pounds of blood will acquire an increment of temperature, every three minutes and a half, amounting to *three quarters of a degree.* How needless the fears of Black, Dumas, &c. that the lungs would be injured by combustion !—that the heat " necessaire pour echauffer le corps, irait a *detruire la substance des parties qui en seraient le foyer.*" How surprizingly does a little calculation dissipate hasty conclusions !

But some will be ready to ask, what ! will these three quarters of a degree, imparted to the blood in the lungs, be sufficient to explain the cause of animal heat, and to account for the continual loss of it from the body ? I answer, certainly ; and at the same time to evince such wisdom of contrivance, that I am lost for expressions to declare my admiration of it. Before I answer the question, by a reference to facts, I might propose the counter-question; why should *thirty-three pounds* of blood circulate through the lungs every *three minutes and a half?* Must *seventy-three gallons* of that fluid rush, every hour, like a torrent through the system, to wash away, and carry off, the *waste* of the body—to carry off *less* than *half an ounce* of matter? Is there any proportion between the means and the end? Why could not a *single* circulation in that time perform the work? why not one that was *diurnal?*—no chemical salt was ever freed from impurities by such a deluge? To these questions no reasonable answer can be given; for is it to be supposed, that the reparation of waste, or glandular secretion, demands a flux of blood through the heart and lungs amounting to *more than twelve hundred gallons daily*? But though the question is beset with irremoveable difficulties when attempted to be answered according to the theory of waste and reparation, it is a question most easy of solution, when viewed in the light of reason. For, let us suppose, that we had had a *diurnal* circulation of the blood, instead of one completed every three minutes and a half—though waste might then have been carried off, and damage repaired—what would have happened to the system? The apprehensions of Black, &c. would *then* have been well founded; for instead of three quarters of a degree imparted to the blood every three minutes and a half, as now happens, not less than *three hundred degrees* of temperature must have been communicated to it!—destruction would have ensued; to say nothing of the unequal distribution of heat to the body. If the most consummate engineer had been consulted, how to preserve a given mass of matter at an *equable* temperature, he could not have suggested any contrivance superior to that of the circulation of the blood. The fluid that warms the body returns for a fresh supply of heat *before it has lost a degree of it.* The capillaries and the lungs are now at the same temperature; and no other arrangement, that we can imagine, could have accomplished the same end. Is this not design? Is it not a master-piece of wisdom?

I have spoken of the heat evolved on the combination of the carbon of the food with the oxygen employed in respiration, because it is of that heat only that I can affirm any thing certain, and appeal to facts to establish it; otherwise, it is not improbable, that the changes which take place in the stomach, as well as those *attending* the liberation of the carbon in the lungs, may either add to, or subtract, from it. But what is speculative I leave for others; and concern myself only with that which admits of demonstration.

Let us now recur to the question I proposed to answer, viz., whether these three quarters of a degree, imparted to the blood in the lungs, be sufficient to explain the cause of animal heat, and to account for the continual loss of it from the body. For if this question be answered satisfactorily, we shall not only admire the wisdom of the contrivance by which that purpose is effected, but shall also be led to the conclusion, that such a purpose must be indispensible in the economy of man, or such a stupendous apparatus would not have been prepared to effect it. And we may further conclude, that the importance of the end answered—animal heat—is proportionable to the greatness of the means employed.

We have seen, that thirty-three pounds of blood receive an increment of three quarters of a degree every three minutes and a-half. Now the capacity for caloric of animal muscle, is but about .75 that of water; and water agrees very nearly with blood; so that every degree in the blood will give 1⅓ of a degree to muscle. Hence, in about three minutes and a half, thirty-three pounds of muscle would receive one degree of heat, supposing none to be lost; and, in about fifteen minutes, a body, weighing one hundred and forty pounds, would receive the same increment; or, in round numbers, four degrees every hour. Now it may be questioned, whether a body, weighing one hundred and forty pounds, *enveloped in clothing*, would—centrally, as well as superficially—lose a greater number of degrees, in that time. If the body be not of the best form for preserving heat, which would be a sphere, it is the next best, a mass of cylinders, which present less surface than any other figure, except the globe. Such a person, momentarily deprived of life, would, *in his clothes*, require something more than ten hours to lose *all* his heat—supposing the loss to be uniform, which would not be the case; though, to spare an unnecessary, scrupulous calculation, it may be so assumed. —My own opinion is, that it would take a considerably longer time, *under such circumstances*, to reduce the body to the temperature of the air. One authority, which meets our present question in some degree, observes, " Animal *heat* is *preserved* in most *sudden* deaths, *much beyond the time* which would be necessary for the dissipation of that which the body, considered as a mass of dead matter, contains at the instant of apparent dissolution." The *duration* of the heat is here admitted; and the *opinion* accompanying it may be classed with the fears of Dr. Black, Dumas, Lagrange, and Hassenfratz, respecting the destruction of the lungs by the generation of it in them. Dr. Ferriar, in his "Treatment of the Dying," remarks—"*Several hours* may be very properly suffered to elapse before the body is laid out; for the joints (central parts) do not, commonly, become rigid for a *considerable time.* At the end of that period the *body will be completely cold.*" It may be observed here, that Dr. Ferriar is speaking of *ordinary* death; *before* which event the body has generally become comparatively cold from the diminished act of respiration. At all events, if we compare together the result deduced from principles, with the facts recorded by observation, we find them most admirably to harmonize, and the certainty of the one to confirm the truth of the other.

But, to decide the question by direct evidence, I had recourse to the following experiments:—In a vessel of water, whose temperature was 65°, I immersed both my hands; having, previously, placed a thermometer in the water.—In fifteen minutes the temperature rose to 70°—during the last five minutes scarcely advancing one degree.—For five minutes, afterwards, it continued, steadily, at the same point. I, therefore, inferred that the heat evolved from my hands—whether radiant, or combined with water—was just sufficient to support the evaporation going on at that temperature. Now the force of vapour at 70°, according to Dalton, is equal to a column of mercury 0·726 inches high. The dew point I found to be 48°; at which temperature the force of vapour is 0·3372. The difference of these is 0·3888; and the quantity of vapour raised from a circular vessel of six inches diameter, by this force of elasticity, is 1·6 grains, per minute. The *area* of the surface of the water in the vessel I employed, was, very exactly, double this; and, therefore, 3·2 grains were dissipated every minute. But the temperature of the room, in which the experiment was made, was 63°; and this, of itself, would have carried off a portion of these 3·2 grains; even if my hands had not been immersed. Let us inquire how much.

The force of vapour at 63° is equal to 0·5749 inches of mercury; from which subtracting the force at the dew point, viz. 0·3372, there remains 0·2377; which would have raised in vapour 1·84 grains in the same time. This subducted from the former quantity, leaves 1·36 grains of water converted into steam by the heat evolved from my hands every minute.

Now the surface of my hands I found—by as accurate a measurement as I could make—to be about the one-sixteenth part of my whole body; and my hands are smaller than those of any man I ever saw. I mention this, because Cruikshanks declared his hand to be only one-sixtieth part of his body; but then, perhaps, he was a subject of twenty stone. Were I to be coated round with some eleven stone more of muscle and fat—dreadful incumbrance! my hand, probably, would not increase one-third of its present *surface;* so that the discrepancy may easily be explained.

It will be remembered that I have stated, that the thirteen grains of carbon combined with oxygen in three minutes and a half's respiration, would generate as much heat as would convert one hundred and forty grains of water into steam. Now, if a man receive into his lungs eight hundred cubic inches of air per minute—as is affirmed—or, by weight, something more than two hundred and forty grains—at the temperature of 60°, and respire them, *saturated with vapour*, at 88°—and this was the *dew point* of my breath this morning—then, since the capacity of air for caloric is to that of water, as 0·2669 to 1; or, nearly, 1 to 4, we should have, from the same quantity of caloric, sixty grains of water heated 28°; equal to rather more than one grain and a half converted into vapour per minute; or five grains and a quarter in three minutes and a half.

According to Saussure, a cubic foot of air, at 66°, is capable of holding eight grains of water; and Leslie affirms, that this quantity doubles for every additional 27° of temperature. By experiment, as I have just mentioned, I ascertained, that the dew point of my breath was 88°. We may, therefore, consider, from the preceding authorities, that fourteen grains and a half of water were contained in every foot of air I respired. Now, if, for the sake of calculation, we assume the average dew point of summer to be 50°—and Dr. Thompson, in his work on *Heat*, &c. says the spring water at Glasgow, whose temperature is 47·75, is sufficiently cold to determine the dew point, so that we cannot greatly err in fixing upon 50°—then, since one cubic foot of respired air, under these circumstances, would contain fourteen grains and a half of water, of which five grains and a quarter existed in the air previous to respiration, there will remain nine grains and a quarter abstracted from the lungs by every cubic foot: or—since a man respires eight hundred cubic inches per minute—we have four grains and a quarter, as the amount of vapour expended in that time; or fifteen grains in three minutes and a half. This, though greater than the quantity found by Dr. Menzies, agrees, very nearly, with the experiments of Abernethy. This quantity of steam, added to that equivalent to the heated air respired, gives us twenty grains and a quarter; which, subtracted from one hundred and forty grains—the whole quantity that could be formed by the thirteen grains of carbon—there remain about one hundred and twenty grains of steam, to account for all the heat lost by the body in three minutes and a half.

Let us now apply these deductions to the experiment above stated. We found that the quantity of vapour raised by the heat of my hands in one minute, was 1·36 grains; or, in three minutes and a half, about five grains; and that the surface of my hands is one-sixteenth of my whole body.—We have also seen, that one hundred and twenty grains is the quantity allowed for the expenditure of the whole body for three minutes and a half. Now one-sixteenth of this is seven grains and a half. This exceeds my experiment by two grains and a half—so that *I* do not lose *so much* heat in three minutes and a half, as would be generated by the combination of thirteen grains of carbon with oxygen gas. This is a very strong verification of my position, that the carbon expended in the lungs is sufficient to explain animal temperature, and the loss of heat by the body. And when it is again told, that *I* do not take more than *two-thirds* the food necessary to furnish eleven ounces and a half of carbon a day, and also that I breathe only *fourteen* times in a minute instead of *twenty*, we shall see, that the heat given out by my body very exactly corresponds with, and confirms, the above theory; for five bear, exactly, the same ratio to seven and a half, that two do to three—*the proportion of my food and respiration, to that assigned by Physiologists; or deduced from their experiments*. I may observe, that, precisely, the same result presented itself when I immersed my feet in water; though the fixed temperature was 80°. It would be superfluous to trouble the reader with all the calculations of it. In the above experiments I, purposely, neglected the little loss arising from the radiation of the vessels. The small difference of temperature between the water and the air, and the little conducting power of the vessels themselves, rendered such a consideration unnecessary.

Hence, then, it is evident, that the communication of *three quarters of a degree* of temperature to THIRTY-THREE POUNDS OF BLOOD, every *three minutes and a-half, is* sufficient toexplain the cause of animal heat, and the continual loss of it by the body. I have many other facts corroborative of this conclusion, as well as of those already drawn in this Essay, which I forbear to introduce here, as exceeding the limits of my design. Should the subject excite sufficient interest, I may embody them in a second edition. The present is but an outline.

Having thus shown that the food we take just furnishes the carbon expended in the lungs during respiration—that this carbon will, on combination with oxygen, just furnish the heat lost by the body—that the sanguiferous apparatus is the most admirable that could be contrived for preserving an equable temperature in it—I conclude, that such a correspondence between the means provided, and the end accomplished, warrants us to believe, as I have before observed, that that end was the design contemplated.

If I saw a steam-boiler, from which ramified a number of pipes, in all directions, through the different apartments of a manufactory, and found all of them filled with a hot fluid—and had previously ascertained that a certain temperature was necessary for the processes going on, or the persons engaged in them—I might as well doubt, whether such an apparatus was intended to warm such a building, as question whether a similar, though incomparably superior arrangement in the animal system, was not the object intended by the Contriver of it.

Perhaps I may be asked, what! does food; do the stomach, the lungs, the heart, with its appendages, serve only to maintain the temperature of the body? Would it not have been a much simpler contrivance to have placed it in a medium in which it could not cool? And is the use of food, or respiration, suspended, when it is surrounded by air, or immersed in water, of its own temperature? As far as *this* Essay is concerned, I need not answer these questions. I have proved, I humbly think, to the strictest demonstration, that the temperature of the body is owing to food—that almost every grain of carbon in it is employed for that purpose; and that an apparatus, perfectly suitable, is contrived, still—although to go fully into this subject would exceed my design—I will add a few observations more respecting animal temperature, which may serve, in part, to answer these questions; reserving a more full exposition to a future Essay, which I may one day publish, on the blood —its constituents and changes—and the fluids secreted from it—with remarks on the mode of glandular secretion, &c.

Why man was not placed in an atmosphere of the same temperature as the body, may easily be answered, when we consider the circumstances of the globe he inhabits. As now constituted, there is no spot on it where he could dwell; for there is no spot where the temperature is, uniformly, equal to that of his body. So that an entirely new arrangement must take place in the economy of the globe to meet this requirement. A coincidence of the planes of the equator and ecliptic would not, as some suppose, give us a perennial season—though, if it would, we have not such a coincidence to afford it—rain must never fall; or water, converted into vapour, must absorb no heat—the winds must be constant in direction and intensity—and man must not set his foot, a single stride, on either side of the parallel of latitude in which he lived; or he would, instantly, encounter a change of temperature. Thus, we see into what extravagancies and perplexities we fall, when we undertake to correct the economy of nature. Truly may we say with the Psalmist, of the works of the Divine Architect, "In wisdom hast thou made them all."

It may further be observed, that the temperature of the human body is placed above the average range of any climate on the globe—no race of men live in a temperature always exceeding 100°—and if he should, at any time, be exposed to heat incompatible with the healthy condition of his body, an adequate contrivance is at hand to prevent any injury resulting from it—the emunctories, like a safety valve, carry off the redundancy, and, consequently, prevent mischievous accumulation—whilst exposure to a lower degree is provided against by clothing; as may be seen in all its variations, from the half-clad Indian, to the fur-wrapped Laplander.

With regard to immersion in a warm fluid, an elegant writer on Physiology observes, "That there is some further purpose to be served, than the mere discharge of carbon for the purpose of producing animal heat, is evident from the consideration, that if an animal be plunged in warm water, so as to have its temperature kept at the natural standard, there is the same necessity for the regular supply of oxygen to carry off the carbon, as under ordinary circumstances."

In the first place, it may be observed, that this is not true; as is known to every one acquainted with warm-bathing. A person immersed in water heated to 98° or 100°, would *not* consume so much carbon as "under ordinary circumstances." The respiration always becomes languid under such circumstances; and if the immersion were continued long, it would occasion syncope. But even admitting that nearly the same supply of oxygen is necessary to carry off the carbon, as under "ordinary circumstances," is that any thing more than might have been expected? Did the writer imagine, that so complex an apparatus as that of the stomach, the lacteals, the heart, the lungs, &c. which were designed for "ordinary circumstances"—for the everyday business of animal life—was, from an *extraordinary* circumstance—dependent on the caprice of man—to lose its office, to stop its functions, till, at his pleasure, he called them again into action? We find, that men placed in a warm temperature, take less *solid* food than those in colder regions. The Coolies, as we have seen, not only subsist, but work hard upon twenty-four ounces of rice a day; whilst a Finlander consumes more than four times that quantity of animal food in the same time. A neglect of this in temperate climates—the consumption, or the endeavour to consume as much food in summer as winter, and *too much at all times*—has carried thousands, and tens of thousands, prematurely, to the grave. If the establishment of this truth, upon scientific principles, were the only consequence of this Essay, I should feel that I had rendered a service to mankind, quite sufficient to justify its composition—but more of this in the sequel.

Upon the other implied part of the question, I shall be very brief in this place. Upon what the energy of the body depends —which, always, in health, succeeds the taking a due proportion of food—I do not here attempt to explain. That the generation of heat is *one* cause, I am quite sure, from my own experience, and the every day facts that obtrude themselves upon our observation.

Blane, in his *Medical Logic*, says, that at 62°, man enjoys the greatest measure of vigor, both in body and mind. This, if true, might justify the inference, that at that temperature the body loses the heat generated in it with the least inconvenience. But animal life always appears more vigorous in a high than in a low temperature. Every summer bears witness to this fact; and no where is life so "abundantly" enjoyed, as by the wild beasts of the torrid zone. Whilst the circumstances of hybernating animals prove, that the absence of heat is, as far as sensibility is concerned, the termination of existence. A few facts, furnished by creatures of this class, will serve to shew the vast importance of temperature to animal life, and vindicate the arrangement that is made for the preservation of it in the human body. It may be remarked, *in* *limine*, that scarcely any large animal is found to hybernate. The bear and the badger relapse into a state which is called "quiescence;" a state very distinguishable from torpescency. One reason, perhaps, why some smaller animals do so, is the over proportion of the surface of their bodies, to the solid contents of them. And it may further be remarked, that in warm climates, even these do not hybernate. In confirmation of which, Dr. Barton informs us, "that in the United States of America, many species of animals which become torpid in Pennsylvania, and other more northern parts of the country, do not become torpid in the Carolinas, and other southern parts of the Continent."

But let us consider a few of the circumstances attendant on hybernation — they will add to the evidence already adduced in proof of the connexion between the consumption of food, the generation of heat, and the possession of vital powers. The dormouse has, during summer, a temperature of 101°, which, in winter, is reduced to 35°. Accompanying this change, is a *total abstinence from food.* The pulsations, which, in summer, are too rapid for calculation, are, in this dormant state, reduced to *sixteen beats in a minute!* The respiration suffers an equally striking diminution. Bats, also, are soon affected by the changes of the atmosphere; and they cease to respire in a medium of 43°. In the month of July, the thermometer standing at 80°, the internal temperature of a bat was found to be 101°. Mr. Cornish applied a thermometer to a torpid bat, and observed that it indicated 36°. The hedgehog, according to Professor Mangili, respires only from five to seven times in a minute during ordinary repose. When it becomes torpid, the process of respiration is, periodically, suspended, and renewed. The marmot has its summer temperature of 102°, lowered to 43°, or less, in winter; and *its respirations from* 500° *to* 14° *in the hour;* and these made at intervals of about four minutes.

In every experiment that was made upon bats, marmots, &c. there was found a certain consumption of oxygen gas, and the evolution of carbonic acid gas, just in the same manner, though proportionably less in quantity, that would have taken place during their active state; and quite sufficient to explain the source of the few degrees of heat, at which their bodies were preserved above the medium in which they were placed. Thus a marmot lost, between the first of December and the fifth of February, about two ounces of its weight, or about thirteen grains daily; a quantity which Dr. Monro ascertained to be lost by a hedgehog under similar circumstances. With this *loss of heat*, there is also a *loss of sensibility*—"*parts of their limbs may be cut off, without the animal shewing any signs of feeling*;" and yet, on the restoration of heat to their bodies, they are, as might be supposed, very susceptible of pain. Heat, therefore, and susceptibility of pain, or sensation, are somehow connected. From all these facts, we discover, that active life, respiration, and its necessary consequence, increased temperature, gradate, co-ordinately, together. I must leave it for others to separate them; and to shew, why they are always associated, if they have no necessary connexion.

Perhaps, from the example cited from Monro, respecting the loss sustained by the hedgehog, the doctrine of *waste* may seem to be established. I do not deny that the absorbents *can* take up parts of the body under particular circumstances; but, surely, if they had been designed to remove "*waste*," they would have had an *outlet in some part of the intestinal canal*, as the readiest channel to carry it off, and not have loaded the circulation with it; especially since the blood is, already, supposed to be supplied with an abundance of *fresh materials* from food. In the instance under consideration, it is the absence of food that brings the absorbents into action. The animal, when it becomes torpid, is always fat; on his revivification he is always lean. His body has been his magazine during the interval. But the very fact, that the absorbents conduct their contents into the circulation, is a proof, that those contents are not intended to be thrown away as "waste." It seems a contradiction of all economy to mix the *waste* and *fresh materials together;* and yet this takes place in an extraordinary way, if that theory be true; for, by *one common tube*—the thoracic duct—the waste and the food enter the circulation! If a person were to see a mill fitted up with stones to grind any hard material, and sieves to separate the finer from the coarser particles; and observed, that all the coarser particles were returned with fresh materials to the stones, *by the same hopper*, to be ground and sifted again, would he ever think that those coarser particles were waste? The miller puts aside his bran, and never mixes it with the wheat that is to be ground; and yet this is the opinion currently received respecting the economy of nature! I know it may be said, that these *débris* are only waste as regard the animal structure; and that, as respects other uses, they are made to supply the carbon of respiration; but this is all conjecture, and is, *diametrically, opposed* to other facts which have been stated in a former part of this Essay.

I will add, in this place, what might have been introduced earlier, a few more remarks on the subject of waste. The absorption of the coloring matter imparted to bones is, as we have seen, supposed to favour this opinion; but to give it any weight—and I think the *striated* appearance observed by Dr. Hunter renders it lighter than the dust of the balance—we must suppose the phosphate of lime to be absorbed also. In *necrosis*, I know, this happens; but that is disease, and we are speaking of the body in health: though a fever is said by some to be but a *vis medicatrix*. Now, if phosphate of lime be absorbed, and carried into the circulation—whether by the lymphatics, or the veins—it is still phosphate of lime; and therefore is, in the blood—according to the theory I am exploding—

mixed with fresh phosphate of lime supplied by the food. Why, I ask, should one portion of phosphate of lime waste away from bone, that another portion of phosphate of lime, *identical with itself,* should go to take its place? The fresh phosphate of lime is no better than the waste phosphate of lime. Chemists know this: so that the old phosphate of lime—after taking an excursion round the system—might go quietly again to its former lodging-place. So beset with absurdities is the doctrine of waste.

But I would ask, concerning the breaking up of the muscles, what new compounds are formed? Some such there must be, because they tell us of *carbon set free,* which blackens venous blood. None of these new substances have been detected; none have been imagined; and yet they must be enormous in quantity to furnish *eleven ounces and a half of* PURE CARBON *every twenty-four hours!* And—to pass over the evolution of pure carbon under such circumstances; and the anomaly of its combination with oxygen gas at the temperature of 100°—why should the carbon of this waste matter unite with oxygen *more readily* than that contained in the food? The *latter, we know*, will, *spontaneously, form such a combination when exposed to air;* as the putrefactive fermentation abundantly proves; but what makes pure carbon enter into such union we have no evidence.

In the course of these inquiries my attention has been drawn to the hitherto irresoluble question, whence herbivorous animals derive their nitrogen. A brief consideration of this question may also serve to throw some light upon the mode of derivation of that element by men; such as the Brahmins, the Coolies before mentioned, and others, who, almost exclusively, live upon vegetable diet. At the same time it will explain away the discrepancies which perplexed Davy, Allen, and Pepys, &c. whilst prosecuting their experiments upon respiration.

The average weight of an ox is said to

be about 660 pounds, of which about 110 pounds are estimated as bone. About fifty pounds of blood are lost when it is killed; which may be disregarded in this calculation. About 100 pounds consist of tallow and fat; thus leaving about four hundred pounds of muscular fibre containing nitrogen. To this we may add the gelatine contained in bone; which, upon the *average*, amounts to forty per cent: the large bones, according to Vauquelin, contain more, though the smaller and more spongy bones contain less. If to one hundred and thirty pounds of dry fibrin—the quantity to which the four hundred pounds would be reduced on exsiccation — there be added these forty pounds of gelatine, then we have one hundred and seventy pounds of animal matter to account for. These one hundred and seventy pounds—the whole of which we will regard as fibrin—will be found to contain about thirty-four pounds of nitrogen; which must have been derived by the animal, from some source or other, in about two years; or at the rate of seven-ninths of an ounce a day, or the thirtieth part of an ounce per hour; or about one-fourth of a grain per minute! This would be equal to five-sixths of a cubic inch only—a quantity so inconsiderable, that it would be wholly inappreciable, when considered in *each* inspiration of so large an animal; it, scarcely, exceeding the one-twentieth of a cubic inch!

Davy concluded, that a *man* consumed, or absorbed, during respiration, a quantity of nitrogen equal to between $\frac{1}{70}$ and $\frac{1}{100}$ of the whole volume of air that entered the lungs. Allen and Pepys state it at $\frac{1}{200}$. Berthollet found it to vary from 0·69 to 3·7 per cent.; and Dr. Bostock declares it to amount, on the average, to $\frac{1}{80}$; which gives about ten cubic inches per minute; a quantity more than *ten times as great as is necessary to furnish the nitrogen contained in the muscular fibre of an ox!* Whether the muscular fibre of man derives its nitrogen from this source I do not pretend to say. I am at present engaged in a course of experiments on the blood; and the result of these may enable us to answer the question. At all events we may look at this source as adequate to explain the formation of muscular fibre, as well as urea, in animals, the Brahmins, the Coolies, and all others —including the disciples of Dr. Lamb,— who subsist on vegetable diet. It is not a little surprising, how a brief calculation removes giant difficulties. We shall, I think, be no longer perplexed to understand whence herbivorous animals derive their nitrogen.

In the course of this Essay I have briefly adverted to animal electricity, and *here*, almost as briefly, I must dispose of that intricate, and little understood branch, if I may so call it, of Physiology. That electricity is much concerned in animal life I am quite convinced. The results obtained by the committee of the French Institute— if we had no other evidence—are sufficient to justify that opinion; whilst the very circumstance that led to the discovery of galvanism, adds great weight to it. It would be foreign to my design to enter fully upon this subject at present; and from the length to which this Essay is already extended, I must suppress the observations I had intended to make. It may, therefore, suffice to observe, that the nerves are good conductors of electricity—that the muscles are brought into action by its agency—that the emunctories of the body are influenced by it—that respiration, as Mr. Read shows, renders the air of our apartments "negative"—that cold, as M. Hemmer observes, does the same to the body—both of which are phenomena connected with the function of the lungs: and when it is remembered, that no chemical change takes place without the development of this agent, we may infer that the processes going on during digestion and respiration both evolve it. We know that silk stockings, when taken off, exhibit strong electricity; derived, probably, from the body—whilst many diseases, as spasms, epilepsy, apoplexy, &c. are, very likely, dependant upon that fluid. The subject is deeply interesting, and deserves further investigation. I have no doubt that respiration, and probably digestion, are accompanied by electrical evolutions; my present experience goes to prove that *positive* electricity is favourable to vital energy; whilst, whatever conducts it too rapidly from the body enervates its powers. Were a Committee formed for prosecuting this subject, I could throw a stone to the cairn.

It is now time to draw this Essay to a close; but, before I do so, it will be well to review the points attempted to be established in it. They are as follows—first, That the opinion of Physiologists respecting the waste of the body is founded in error. Secondly, how *all* the food received into the stomach is eliminated from the system. If the quantity of carbonic acid gas formed in the lungs, and all the substances secreted by the kidnies, and those which pass off by the exhalants, were added together, it is probable, that not one grain would be found either in excess or defect—our calculations have brought them so near to this, as to warrant the conclusion. Thirdly, we have seen that all the carbon expended in the lungs is sufficient, and *just sufficient*, to maintain the temperature of the body; and we have shown that the apparatus fitted up for this purpose is, precisely, what a consummate engineer would himself erect. Fourthly, it appears, that though "growth," which depends upon nutrition, is provided for in the animal economy, still, small as the quantity required is, it puts nature to great efforts; and yet under other circumstances, the quantity of food demanded and expended, is, considerably more variable; because the very demand carries off the carbon, and sets the oxygen and hydrogen free for escape by secretion, exhalation, &c. which the fœtus does not. Fifthly, the apprehensions of Black, &c. respecting the injury to which the lungs would be exposed if heat were evolved in them, have been satisfactorily exploded; and, lastly, the source whence nitrogen is derived by herbivorous animals, has been explained upon principles that before perplexed the greatest physiologists and chemists: I mean the unaccountable loss that appeared in their experiments upon respiration. Whatever part of these conclusions is not founded upon demonstration let it be rejected; hypothesis may, sometimes, be a useful scaffold with which to erect the fabric of knowledge; but when once the building is finished it ought to be taken down—the scientific edifice should rest on the foundation of truth only. The humble structure I have attempted to raise, did not need such a scaffold—I have had no other merit than that of placing the bricks, and spreading the mortar, which others have prepared.

I will now add a few words, in answer to the *cui bono* question, that may be made respecting the doctrine of this Essay. If I could shew no immediate application of it to the purposes of life—if it stood a naked series of truths, unassociated with any branch of science, still there is no truth, however inconsiderable it may appear at first, that should be disregarded. Time may find for it uses, of which, at present, we have no conception. The contraction of a muscle, on the application of metallic wires—the friction of a piece of amber—the accidental arrangement of two pieces of glass—the vibration of a chandelier, were the germ of distinct branches of science, or the first step towards most valuable discoveries. But when—as in the present instance—the truth is intimately connected with the economy of animal life—with man himself—when it is known that the most important functions of that economy, are dependent upon it, surely the truth must more deeply interest us—we cannot regard it with indifference. One fact I have already mentioned, which no one can neglect with impunity; to proportion our food to the consumption of it; and not because fortune has prepared a banquet for us, and custom has taught us to sit down at stated times to partake of it, to indulge beyond the demands of nature. A violation of this canon she almost always punishes. Let not the enervated female, who, from inaction, cherishes debility, hope to remove it by the delicacies of the table; nor the student, who feels his nerves relaxed from sedentary habits, think to relieve himself by a full meal,—What the stomach does not digest—and it cannot long digest more than is expended—will visit the bowels with constipation, and load the blood with redundant matters, which can only find their exit by eruptive disease, or congestion, inducing an inflammatory diathesis. I would, strongly, advise every one, who wishes to enjoy life, to ascertain the *minimum* of food upon which he can subsist. Should he err a little on that side, nature will kindly admonish him of it; if he err in the other extreme, she will punish him.

Again, the active powers of man have been shewn to depend on animal temperature; and this temperature has been shewn to increase with exercise—the lungs then consume more carbon. Let those, therefore, who complain of cold extremities, not hope to warm them with wine, but by increasing the pulmonary fire—by the free use of their limbs. And let exercise be exercise; for many walk with as much caution and deliberation, as if they were afraid of crumbling the fabric to pieces by motion.

To those for whom fortune has not prepared a luxurious table, I say, be thankful to a wise and gracious Providence, that has furnished the meanest fare with as much carbon, as the rarest delicacies. And let all be moderated in their pride, when they sit down to refresh nature, by the thought, that they are then only putting coals upon the fire, to keep the machinery of life in action. In this view, what a humiliating picture does the most sumptuous entertainment present!

If any professional man should read this Essay, and feel any qualms about the conclusions drawn in it, I beg to request of him to satisfy himself upon the following particulars, before he passes judgment. Are eleven ounces and a half of carbon consumed in respiration during twenty-four hours? And do thirty-five ounces of bread, or about twenty-nine ounces of bread, and between twelve and fourteen ounces of meat, just furnish that quantity? Do the kidneys secrete three pounds of fluid a day, consisting of substances which, exactly, contain the nitrogen found in these twelve or fourteen ounces of meat? Will eleven ounces and a half of carbon, on combination with oxygen gas, impart four degrees of temperature, per hour, to the whole system, supposed to weigh one hundred and forty pounds? And does the body lose more heat than this in the same time? And can any other method be devised, by which an *equable* temperature could be so admirably preserved? Does not the energy of the body—within certain limits—evidently depend upon this temperature? How can I prove that the body wastes? What new arrangements are made out of the waste elements? How do they get into the circulation? and why, when there, since they are composed of the *same* elements as the fibres that remain, do they not go again to repair that waste? And why does waste take place more rapidly during digestion? Why does the lion waste four times as fast as man, even when he lives in a state of inactivity? If he can answer all these questions differently from those contained in this Essay, he will not only destroy all my conclusions, but scatter to the winds the labours of the greatest men that ever adorned the annals of science. To me I hope he will deal with a gentle hand, seeing that I write for men of my own size; and have only been drawn to these speculations from a love of truth, and a desire to benefit—however slender my effort—the generation in which I live. If he foster my attempt, I may be led to venture another step into the arcana of nature's economy—the changes sustained by the blood in its circulation—glandular secretion—the nature and cause of inflammatory diathesis, —and the cause of some of the most obscure diseases to which the system is liable. If he frown upon my design, why then I can confine myself to other pursuits more commensurate with my capacity.

I will add one word more for the general Reader. About thirty ounces of bread, or substances composed of the same elements, and twelve ounces of meat a-day, have been shown to be amply sufficient for a man who weighs ten stone, and who *breathes twenty times in a minute.*—My own consumption of food, and that of my children, is not equal to *half* that quantity; and we enjoy almost unexampled health. For thirty years I have never once been confined to a sick-bed; and what indisposition means, they only know by report. A severe inflammatory fever, caused by exposure to cold, wet, and a keen east wind, which confined me at home several weeks in the spring of the present year, yielded to diet and regimen alone—and I am quite sure, that *twenty ounces of food a day*, are the *maximum* for *me*. I, frequently, take exercise to the extent of seven miles before dinner, on a winter's day, that shuts the forty-ounce-eaters within doors. But, then, I breathe only fourteen times in a minute; and, probably, on the average, not so often. Let it also be remembered, that two ounces of excess, in the case of Cornaro,—or fourteen ounces a-day, of solid food,—produced a pleurisy, in a fortnight. Of ale, wine, and spirits, I take none; or, at most, not twice a-year; and then not more than a glass. When I need them "for my stomach's sake, or often infirmity," I shall not scruple to use them. Though a friend to temperance, I know of no authoritative command to observe total abstinence. A higher principle than a "pledge" is, I think, necessary to make us "moderate in all things"—at all times. They who will not listen to the Word that cannot err, may, perhaps, be led to restrain excessive indulgence by the consideration, that it ruins health, enervates the mind, makes the heart fretful, and casts a gloom over every prospect of life; whilst *to live on twenty ounces* *of food a-day will correct all these.* But, then, the system must not be deluged with diluents, or inflamed with wine. Twenty ounces of tea, or coffee, a day, will suffice for nature; and more will only relax the body. I sometimes err in this, and am punished. If any one shall be led to make trial of this advice—founded upon the reasoning in this Essay—he, at least, will, ever after, acknowledge his obligation to me for writing it.

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